

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Racial and socioeconomic disparities in patient experience of clinician empathy: a protocol for systematic review and meta-analysis
AUTHORS	Roberts, Brian; Trzeciak, Christian; Puri, Nitin; Mazzairelli, Anthony; Trzeciak, Stephen

VERSION 1 – REVIEW

REVIEWER	Louis Penner Wayne State University/Karmanos Cancer Institute, Oncology
REVIEW RETURNED	17-Dec-2019

GENERAL COMMENTS	<p>Bmjopen-2019-03427</p> <p>The protocol described in this submission seems a bit narrow but nonetheless probably of sufficient interest to researchers and practitioners who are interested in how patient race and SES might patient experiences of empathy. The procedures for conducting this review seem consistent with standard practices for such efforts and the authors seem quite familiar with the kinds of things they will be required to do.</p> <p>The databases that will be searched seem to be comprehensive and appropriate to the focus of this review.</p> <p>The data analyses seem to be the correct one for the task at hand, but I do some suggestions that might improve the review. First race needs to be clarified; what “races” are to be included? Will ethnicities (e.g., Latainx) that are technically not a race in the common usage of the term be included as separate categories? In other words, more specificity in “race” is needed. Second, race has value as a descriptive term to describe some current situation but essentially none as an explanatory one. That is, it is not a patient’s race that would explain perceptions of empathy but rather it is things that would covary with race. My own work suggests that racial attitudes among African American patients might play a significant role in perceived empathy, but obviously these would not be available in many of the articles reviewed, But the authors will be getting information on SES which strongly covaries with racial self-identification. It would not be much of an additional burden to look at the effects of SES within each race in the analyses or even formally examine the interaction between the two. I feel pretty strongly about this because studies that only consider a patient’s race certainly have value but may contribute to the implicit assumption that “race” per se causes healthcare disparities. Race is a social construct and by itself explains almost nothing.</p>
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REVIEWER	Carma Bylund University of Florida College of Journalism and Communication
REVIEW RETURNED	29-Jan-2020

GENERAL COMMENTS	<p>The authors should make a stronger case for why they are doing a rapid systematic review rather than a regular systematic review. I understand that they are building off a recent one, but that doesn't explain why they think a rapid one makes more sense.</p> <p>Since the protocol has been submitted already to Prospero, what is the added value of publishing it in a peer-reviewed journal?</p> <p>I don't understand the specified focus on "biomedical" literature and how exactly that is being defined. Much of the literature on empathy and empathic communication is published in behavioral science journals.</p> <p>Do the authors feel that there should be a distinction made between empathy and empathic communication?</p>
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REVIEWER	Dr Michelle M Haby Universidad de Sonora, Mexico The University of Melbourne, Australia
REVIEW RETURNED	28-Feb-2020

GENERAL COMMENTS	<p>This is an interesting protocol on an interesting topic. However, the use of rapid review methodology (instead of a full systematic review) is not sufficiently justified. Further, the reporting of the methods suggests major problems with the design of the review that need to be addressed.</p> <p>Major comments:</p> <ol style="list-style-type: none"> 1. At no stage do the authors justify why they are proposing to undertake a rapid review rather than a full systematic review for this question. In fact, they have misquoted their main supporting methodological paper in relation to why a rapid review is needed (reference 30) – page 6, line 46-48. The framework quoted states that the review must have a clearly formulated research question, “and a clear rationale for the needs of the research to inform decision-making. They should also, when possible, involve policymakers, patients, and the public in defining and/or refining the research question.” Neither of these criteria are met for this review. While the authors have not quoted any other important papers in the area of rapid review methodology, they are consistent in stating that rapid reviews are done (in place of a full systematic review) to inform decision-making and/or with the involvement of decision-makers. 2. The authors also note that an important requirement of conducting a rapid review is the inclusion of experienced systematic reviewers. To support their claim, they cite 8 of their own previous systematic reviews (references 32-39). I did not revise each of these reviews but, based on their explanation of the methods that they will use for the current rapid review, I am left with doubts about the level of their experience. I do note, that none were conducted as part of the Cochrane Collaboration.
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	<p>3. Some specific points that suggest a lack of knowledge of systematic review methods include:</p> <p>a. Not clearly defining the types of studies that are eligible for the review (page 8). Based on their choice of risk of bias tool and the nature of the review I assume that they will include observational designs but am not sure if all observational designs will be included. For example, will case series meet the criteria?</p> <p>b. Choosing to search Cochrane CENTRAL for studies. While this is a recommended source of studies for reviews of interventions, given that it includes mostly randomized controlled trials and controlled trials, it is not suitable as a source of observational studies.</p> <p>c. Not stating whether they will include grey literature or not.</p> <p>d. Stating that the protocol was designed in accordance with the PRISMA statement (lines 9-13, page 6). This statement is a reporting guideline, not a methodological guide.</p> <p>e. Having a search strategy (Box 1) that is very limited in that it is less sensitive than used for the original review and does not clarify which fields will be searched for each of the terms. Did the authors seek help from a health librarian / information specialist in designing the search strategy?</p> <p>f. Not specifying which software will be used for reference management, data abstraction, and for analysis.</p> <p>g. Presenting information for the first time in the analysis section (page 10, lines 16-27) that should have first been mentioned in the data extraction section: "We will table: (1) country of origin; (2) clinical context (e.g. primary care); (3) number of study sites; (4) number of clinicians in the study; (5) total number of patients; (6) number of patients stratified by race;... measure data stratified by SES."</p> <p>h. There is conflicting information in the methods. On page 7, lines 3 to 10 the authors state: "...[2] full text manuscripts reviewed by two independent reviewers and data extraction verified by a second reviewer; and [3] all risk of bias assessment verified by a second reviewer.30" but on page 9, lines 3-18 they state that: "Two members of the research team will independently screen the titles and abstracts of identified studies for potential eligibility... Two reviewers will independently abstract data using a standardized data collection form. Any disagreements in these processes will be resolved by consensus with a third reviewer."</p> <p>i. The risk of bias tool chosen (the Newcastle-Ottawa Scale) is appropriate for case-control and cohort studies. The authors have not provided sufficient justification for using it for their review. Further, the authors state that they "customized" the scale but the resulting instrument is a very substantial modification of the original tool!</p> <p>4. Systematic review protocols do not generally include a discussion section.</p> <p>Minor comments:</p>
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	5. Page 2, line 53. The protocol registration number should be added as the protocol was registered in 2019.
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VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1

Reviewer Name: Louis Penner

Institution and Country: Wayne State University, USA

Please leave your comments for the authors below

The protocol described in this submission seems a bit narrow but nonetheless probably of sufficient interest to researchers and practitioners who are interested in how patient race and SES might patient experiences of empathy. The procedures for conducting this review seem consistent with standard practices for such efforts and the authors seem quite familiar with the kinds of things they will be required to do. The databases that will be searched seem to be comprehensive and appropriate to the focus of this review.

RESPONSE: Thank you very much.

The data analyses seem to be the correct one for the task at hand, but I do have some suggestions that might improve the review. First race needs to be clarified; what “races” are to be included? Will ethnicities (e.g., Latinx) that are technically not a race in the common usage of the term be included as separate categories? In other words, more specificity in “race” is needed.

RESPONSE: We agree. We will collect both race and ethnicity information, as described in the manuscripts, for all patients enrolled in the identified studies. If clarification is needed, including clarification for combining data for populations according to race or ethnicity, we will send author queries to the investigators. This systematic review will use the race/ethnicity categories typically used for human subjects research sponsored by the National Institutes of Health (NIH). An example table for NIH-sponsored data collection on race/ethnicity (“Inclusion Enrollment Report”) appears below and here: <https://grants.nih.gov/grants/funding/2590/enrollmentreport.pdf>

PART A. TOTAL ENROLLMENT REPORT: Number of Subjects Enrolled to Date (Cumulative) by Ethnicity and Race				
Ethnic Category	Females	Males	Sex/Gender Unknown or Not Reported	Total
Hispanic or Latino				**
Not Hispanic or Latino				
Unknown (Individuals not reporting ethnicity)				
Ethnic Category: Total of All Subjects*				*
Racial Categories				
American Indian/Alaska Native				
Asian				
Native Hawaiian or Other Pacific Islander				
Black or African American				
White				
More Than One Race				
Unknown or Not Reported				
Racial Categories: Total of All Subjects*				*
PART B. HISPANIC ENROLLMENT REPORT: Number of Hispanics or Latinos Enrolled to Date (Cumulative)				
Racial Categories	Females	Males	Sex/Gender Unknown or Not Reported	Total
American Indian or Alaska Native				
Asian				
Native Hawaiian or Other Pacific Islander				
Black or African American				
White				
More Than One Race				
Unknown or Not Reported				
Racial Categories: Total of Hispanics or Latinos**				**

* These totals must agree.
** These totals must agree.

Second, race has value as a descriptive term to describe some current situation but essentially none as an explanatory one. That is, it is not a patient's race that would explain perceptions of empathy but rather it is things that would covary with race. My own work suggests that racial attitudes among African American patients might play a significant role in perceived empathy, but obviously these would not be available in many of the articles reviewed. But the authors will be getting information on SES which strongly covaries with racial self-identification. It would not be much of an additional burden to look at the effects of SES within each race in the analyses or even formally examine the interaction between the two. I feel pretty strongly about this because studies that only consider a patient's race certainly have value but may contribute to the implicit assumption that "race" per se causes healthcare disparities. Race is a social construct and by itself explains almost nothing.

RESPONSE: We agree. We acknowledge that empathy assessments by patients are a function of not only how clinicians treat patients, but also patient perceptions of empathy. We understand that, biologically, race does not explain differences in perceptions of clinician empathy, and it is the social construct around race instead. Accordingly, covariates with race (e.g. SES) may explain differences in perceptions of empathy. Analyzing the interaction between race and SES is an excellent suggestion, and, where possible, we will compare CARE measure scores between SES categories stratified by race. We describe this in the revised methods and discussion section. [page 9, first para; page 14, first para]

Thank you very much for your continued attention to our manuscript.

Reviewer: 2

Reviewer Name: Carma Bylund

Institution and Country:

University of Florida

U.S.A.

Please state any competing interests or state 'None declared': None

Please leave your comments for the authors below

The authors should make a stronger case for why they are doing a rapid systematic review rather than a regular systematic review. I understand that they are building off a recent one, but that doesn't explain why they think a rapid one makes more sense.

RESPONSE: In accordance with the suggestions of another reviewer, we revised the research plan to a conventional (i.e. regular) systematic review rather than a rapid review. The methods section is modified accordingly.

Since the protocol has been submitted already to Prospero, what is the added value of publishing it in a peer-reviewed journal?

RESPONSE: The value of submitting the protocol paper to a peer-reviewed journal are the valuable insights of the peer reviewers, and the peer reviewer suggestions to make the research as rigorous as possible. For example, in this case the peer review process led us to a substantial change in approach (i.e. conventional systematic review rather than rapid review). The other benefit is transparency for the full protocol, as some people are not familiar with searching in PROSPERO.

I don't understand the specified focus on "biomedical" literature and how exactly that is being defined. Much of the literature on empathy and empathic communication is published in behavioral science journals.

RESPONSE: You are correct, and we have removed the term "biomedical" throughout, and in most cases replaced it with "scientific".

Do the authors feel that there should be a distinction made between empathy and empathic communication?

RESPONSE: This is an excellent point for consideration. We will analyze studies that used the Consultation and Relational Empathy (CARE) measure, an assessment of patient perception of clinician empathy in the broad sense. Therefore, we do not think a distinction between empathy and empathic communication is possible. But this point is important and we modified the limitations section of the discussion accordingly [page 14, second para]

Thank you very much for your continued attention to our manuscript.

Reviewer: 3

Reviewer Name: Dr Michelle M Haby

Institution and Country:

Universidad de Sonora, Mexico

The University of Melbourne, Australia

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

This is an interesting protocol on an interesting topic. However, the use of rapid review methodology (instead of a full systematic review) is not sufficiently justified. Further, the reporting of the methods suggests major problems with the design of the review that need to be addressed.

RESPONSE: Thank you. In accordance with your points of critique, we have revised the methodology to a conventional (i.e. full) systematic review rather than a rapid review, and we submitted a protocol revision to PROSPERO to reflect this change in approach. The change to a full systematic review is now reflected throughout the revised manuscript.

Major comments:

1. At no stage do the authors justify why they are proposing to undertake a rapid review rather than a full systematic review for this question. In fact, they have misquoted their main supporting methodological paper in relation to why a rapid review is needed (reference 30) – page 6, line 46-48. The framework quoted states that the review must have a clearly formulated research question, “and a clear rationale for the needs of the research to inform decision-making. They should also, when possible, involve policymakers, patients, and the public in defining and/or refining the research question.” Neither of these criteria are met for this review. While the authors have not quoted any other important papers in the area of rapid review methodology, they are consistent in stating that rapid reviews are done (in place of a full systematic review) to inform decision-making and/or with the involvement of decision-makers.

RESPONSE: As above, in response to your comments we revised the methodology to be a conventional (i.e. full) systematic review rather than a rapid review, and this is now reflected throughout the revised manuscript.

2. The authors also note that an important requirement of conducting a rapid review is the inclusion of experienced systematic reviewers. To support their claim, they cite 8 of their own previous systematic reviews (references 32-39). I did not revise each of these reviews but, based on their explanation of the methods that they will use for the current rapid review, I am left with doubts about the level of their experience. I do note, that none were conducted as part of the Cochrane Collaboration.

RESPONSE: Our prior systematic reviews have been published in high impact journals such as *Lancet Respiratory Medicine* (impact factor 22.992), *Critical Care Medicine* (impact factor 6.971), and *Resuscitation* (impact factor 4.572), and other high quality journals (e.g., *Shock*, *PLoS One*, and *BMJ Open*). Although we have not previously worked under the Cochrane Collaboration, we believe our previous publication history of systematic reviews underscores our significant level of experience. Further, we now agree that our original methodology to use a rapid review was not in line with the historical context of rapid reviews, and based on your feedback we have modified our approach to a full systematic review as described above.

3. Some specific points that suggest a lack of knowledge of systematic review methods include:

a. Not clearly defining the types of studies that are eligible for the review (page 8). Based on their choice of risk of bias tool and the nature of the review I assume that they will include observational designs but am not sure if all observational designs will be included. For example, will case series meet the criteria?

RESPONSE: Defining the types of studies eligible for review is accomplished through the use of inclusion criteria or exclusion criteria. In our initial submission we used multiple exclusion criteria, and our aim was to keep our search as broad as possible. You are correct that the vast majority of studies will in fact be observational designs. The reason why we did not make observational design an inclusion criterion is because, for example, there may be interventional studies that report data for the CARE measure and we do not want to lose these studies. For the example you gave (case series), the answer is yes – a case series would be included if they reported data for the CARE

measure. We now describe this in more detail in the revised methods section [page 6, under “eligibility criteria”].

b. Choosing to search Cochrane CENTRAL for studies. While this is a recommended source of studies for reviews of interventions, given that it includes mostly randomized controlled trials and controlled trials, it is not suitable as a source of observational studies.

RESPONSE: As stated above, you are correct that the vast majority of studies will have an observational design, but there also will be some interventional studies (e.g. clinical trials) that contain CARE measure data. In fact, we are already aware of a few clinical trials that captured the CARE measure. We do not want to lose these studies, and therefore we believe CENTRAL is a suitable source. As per your suggestion in part “e” below, we consulted a health librarian / information specialist on this specific question and the recommendation was to search CENTRAL as well, for the reason above.

c. Not stating whether they will include grey literature or not.

RESPONSE: We will not include grey literature. We will only include published studies. We now clarify that in the revised methods section.

d. Stating that the protocol was designed in accordance with the PRISMA statement (lines 9-13, page 6). This statement is a reporting guideline, not a methodological guide.

RESPONSE: Yes, we know that PRISMA-P is a reporting guideline for protocols. What we meant in using the word “designed” is that when writing this protocol we were very deliberate to make sure that we included all of the reporting items suggested in the PRISMA-P checklist. We clarified this in the revised manuscript [page 6, first para]

e. Having a search strategy (Box 1) that is very limited in that it is less sensitive than used for the original review and does not clarify which fields will be searched for each of the terms. Did the authors seek help from a health librarian / information specialist in designing the search strategy?

RESPONSE: In the revised manuscript, we use a search strategy that exactly mirrors (verbatim) what Howick *et al* used in the previously published study. It appears below.

MEDLINE (and adapted for searching other databases)

1. "consultation and relational empathy".mp.
2. (CARE adj3 (measure* or question* or index*)).ti,ab. and empath*.mp.
3. (CARE adj3 (measure* or question* or index*)).ti,ab. and mercer.af.
4. 1 or 2 or 3

We clarified this in the revised manuscript [page 7, middle].

Per your suggestion, we consulted with a health librarian / information specialist who agreed that this search strategy is appropriate. Our health librarian’s judgment is that, although many systematic reviews use a search strategy with more detail, the search strategy shown above from Howick *et al* is acceptable and sufficient because “*there is really no other way to say Consultation and Relational Empathy (CARE)*”. Further, our health librarian tested this search strategy by making modifications to the search terms above – to see if any additional studies would be identified. The modifications only identified 11 additional studies, and none of those studies would meet the inclusion/exclusion criteria for our systematic review. Therefore, we the investigators, and our health librarian / information

specialist, have confidence that adopting the search strategy above from Howick *et al* is sound methodology.

f. Not specifying which software will be used for reference management, data abstraction, and for analysis.

RESPONSE: We use Endnote X9 for reference management, Google sheets for data management, and STATA 16 for analysis. We added this information to the methods section in the revised manuscript.

g. Presenting information for the first time in the analysis section (page 10, lines 16-27) that should have first been mentioned in the data extraction section: “We will table: (1) country of origin; (2) clinical context (e.g. primary care); (3) number of study sites; (4) number of clinicians in the study; (5) total number of patients; (6) number of patients stratified by race;... measure data stratified by SES.”

RESPONSE: We moved this to the data extraction section.

h. There is conflicting information in the methods. On page 7, lines 3 to 10 the authors state: “...[2] full text manuscripts reviewed by two independent reviewers and data extraction verified by a second reviewer; and [3] all risk of bias assessment verified by a second reviewer.³⁰” but on page 9, lines 3-18 they state that: “Two members of the research team will independently screen the titles and abstracts of identified studies for potential eligibility... Two reviewers will independently abstract data using a standardized data collection form. Any disagreements in these processes will be resolved by consensus with a third reviewer.”

RESPONSE: We reconciled this in the revised manuscript.

i. The risk of bias tool chosen (the Newcastle-Ottawa Scale) is appropriate for case-control and cohort studies. The authors have not provided sufficient justification for using it for their review. Further, the authors state that they “customized” the scale but the resulting instrument is a very substantial modification of the original tool!

RESPONSE: We believe it is important to include a risk of bias tool. Of the risk of bias tools available, we selected the Newcastle-Ottawa Scale because (as you point out) it is appropriate for cohort studies, and the vast majority of studies in our systematic review will be cohort studies. In the revised manuscript, we scaled back our customization based on the concern you raise above. Our customizations are now just filling in the blanks on the tool originally developed by Professor Wells and colleagues (available at: http://www.ohri.ca/programs/clinical_epidemiology/oxford.asp). See supplementary material for the revised tool we will use.

4. Systematic review protocols do not generally include a discussion section.

RESPONSE: This specific journal (BMJ Open) does permit discussion sections for systematic review protocols. We have done this before: BMJ Open 2018;8(9):e024320. doi: 10.1136/bmjopen-2018-024320. PMID: 30224405

Minor comments:

5. Page 2, line 53. The protocol registration number should be added as the protocol was registered in 2019.

RESPONSE: The protocol registration was still being processed at the time of submitting this protocol to the journal (September 11, 2019). The protocol registration number is now available, and we added it to the revised protocol.

Thank you very much for your continued attention to our manuscript.